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Dr. Robert Ballentine
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Dear Bob,

I am trying to track down whatever documentary history might still be preserved about the perceptions of biologists during the first few years after the publication of Avery's report on pneumococcus transformation. I suspect you may have seen Gunther Stent's piece on this in the Scientific American last December, which does not fairly present the picture upon my own recollection of it. Columbia, Department of Zoology may have been one of the main arenas of debate; and while I can certainly recall very vividly some of the coolness with which chemical approaches to genetics was received among some quarters there, I would hardly interpret that as a lack of perception as to the issues that were being raised. How be it, I just wonder if you happened to have preserved any papers, lecture notes, letters or other material that can be dated in the interval between 1943 and 1947 and which would bear on this question.

In particular I am very curious to try to save what can be about Francis Ryan's role in this and also in the initiation of my own work on bacterial recombination. The "bull-session" that is mentioned in the enclosed is not a matter of specific recollection on my part, and I am having difficulties finding any pieces of paper on which is recorded anything very exact about the intellectual sources of the experiments that are indeed preserved in notebooks from July 1945 on. I know that Harriett Taylor showed me the Avery paper in January 1945 and that we were very excited about it, but I have no actual record of Francis' reactions, or yours, nor in fact in general what put Harriett on to taking such a deep interest in that work -- which, of course, led in due course to her taking a post-doc at Rockefeller later on.

Anything you can throw into the pot would be most welcome; and it would be nice to hear from you again.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr

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